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# Measuring Patterns of Acquaintanceship

by Peter D. Killworth, H. Russell Bernard, and Christopher McCarty

OUR RESEARCH IS DESIGNED to look for the rules that govern an aspect of social structure we consider important: who knows whom, and how. Global social structure may be conceived as a square network, totally connected, of some four billion nodes. Such a network is clearly beyond any individual's comprehension. Indeed, it is an open question just how large a network anyone can comprehend and, consequently, manipulate. Other fields have similar problems. To an oceanographer, for example, most of the surface water of the globe is a single body; yet oceanographers rarely consider it as a collection of undifferentiated molecules, for the simple reason that such a mass is incomprehensible to them. The problem for

both anthropologists and oceanographers, then, is to define the relevant boundaries of the phenomena in which they are interested and to apply useful instruments for describing those phenomena with the goal of eventually producing a theory that explains them.

Furthermore, oceanographers are incapable of handling even a very limited patch of ocean as a collection of molecules. Instead, they combine the complicated effects of the quantum mechanics of each molecule into traditional continuum mechanics. This averages over many millions of molecules (whose behavior may differ violently over tiny length scales) to produce descriptions (and a theory) relating to length scales larger than, say, a micrometer. This is not to imply that oceanographers reject quantum mechanics (and hence the study of molecules), merely that they are interested in larger-scale phenomena. Some anthropologists are interested in very small-scale human relations (e.g., mother-child relations), while others focus on the societal level (e.g., warfare) or above. In all cases there are three things investigators would like to know: what the phenomenon looks like (description), what causes it (theory), and what it causes (prediction).

Oceanography has passed through the fundamental phase of pure description; anthropology has not. Our research is an attempt at working towards a theory of social structure based upon replicable measurement. Unfortunately, we do not know what to measure, or which quantities are more important for understanding social structure: are kinship relations "more fundamental" than, say, debt relations? If one wants to understand ocean circulation, it turns out that velocity, temperature, and salinity are more fundamental than, say, color, no matter how important color may seem at first glance. In other words, knowledge of the appropriate physics has given oceanographers a decision procedure for eliminating irrelevant quantities. (Ocean color is important for many things but not for understanding circulation.) In anthropology, no such identification of the relevant phenomena has yet occurred, nor do we know what form the appropriate physics might take. The quantities involved are certainly not obvious, and they may not be responsive to easy methods of measurement. We are studying the ways (or a way, possibly wrong) one might set about discovering the building blocks for a future social theory. Theories of social structure will require a variety of building blocks, just as theories of oceanography require more than, say, temperature. Since we do not as yet understand the physics of social relations, we must start somewhere; we believe (from the work of Homans 1950, Radcliffe-Brown 1957, Firth 1963 [1951], and others) that social structures are built up—at least in part—from the social relations of individuals (see Killworth and Bernard 1978, Bernard, Killworth, and McCarty 1982).

Obviously colleagues will differ on which aspects of social

PETER D. KILLWORTH is Senior Research Associate in the Department of Applied Mathematics and Theoretical Physics, University of Cambridge (Cambridge CB3 9EW, U.K.). Born in 1946, he was educated at Cambridge (B.A., 1967; M.A., 1971; Ph.D., 1972) and has done postdoctoral research at Scripps Institution of Oceanography in La Jolla, Calif. His research interests lie in both oceanography and anthropology, in the former centering on climate, oceanic eddies, convection, and frontal instabilities, in the latter on the cautious attempt to bring some vaguely quantitative methods to bear on the "glue" that binds people together. His publications include "Deep Convection in the World Ocean" (*Review of Geophysics and Space Physics* 21:1-26), "Absolute Velocity Calculations from Single Hydrographic Sections" (*Deep-Sea Research* 30:513-42), and, with H. R. Bernard and (for pts. 4 and 5) L. D. Sailer, a series of articles entitled "Informant Accuracy in Social Network Data" (*Human Organization* 35:269-86; *Human Communications Research* 4:3-18; *Social Networks* 2:19-46, 191-218; *Social Science Research* 11:30-66).

H. RUSSELL BERNARD is Professor of Anthropology at the University of Florida. He was born in 1940 and educated at Queens College (B.A., 1961) and the University of Illinois (M.A., 1963; Ph.D., 1968). He has taught at Washington State University (1966-72) and at West Virginia University (1972-79) and was a research associate at Scripps Institution of Oceanography in 1972. His research interests are social network analysis, native literacy, technology and social change, and maritime societies. He has published, with coeditor Periti Peltó, *Technology and Social Change* (New York: Macmillan, 1972) and, in collaboration with Jesús Salinas Pedraza, *The Otomi* (Albuquerque: University of New Mexico Press, 1978).

CHRISTOPHER MCCARTY is a graduate research assistant in the Department of Anthropology of the University of Florida. Born in 1958, he received his B.A. from West Virginia University. His interests are Third World development, Eastern Europe and Latin America, and quantitative methods. He has published, with Aaron Podolefsky, "CATS: Computer-Assisted Topical Sorting in the Analysis of Qualitative Data" (*American Anthropologist* 85) and, with Bernard and Killworth, "INDEX: An Informant-Defined Experiment in Social Structure" (*Social Forces* 61:99-133).

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structure are important. Any of the forms of human relations will yield a set of observable, interesting phenomena. The presence of a relationship between two persons presumes that they know each other, or know of each other's existence. Examination of how people know each other is a straightforward way of looking for the rules that govern interactions in a network. The pattern of these interactions—that is, the particular structure—may or may not be an important topic for study; the rules that govern the structure surely must be.

We shall begin by describing a small informant-defined experiment that provided information for the construction of an instrument for describing acquaintanceship. Next we shall introduce the instrument and discuss its application to a study of the acquaintanceship of 40 informants. Finally, we shall examine the implications of our findings.

## THE INFORMANT-DEFINED EXPERIMENT

Most social surveys are constructed to elicit data from informants about topics of interest to a researcher. Our purpose is to find out whom people know and how they know them. In any given culture, for example, do informants need to know the religion of another person in order to feel that they know that person? Or that person's clan? Or what the person does for a living? In other words, we are interested in what our informants find pertinent to their relations with others rather than what we as anthropologists might consider germane. INDEX is an interview procedure designed for eliciting this information, which can then be used in the more formal procedure to be described later in the paper.

We told 15 paid informants in Gainesville, Florida, that we had complete life histories of 50 persons around the United States and 10 persons from elsewhere in the world. In keeping with the jargon, these persons are henceforth termed "targets" (see Milgram 1967 or Bernard and Killworth 1979 for a review). These 60 targets were mythical, but we had invented "life histories" for them to make them plausible. We selected information about targets (occupation, religion, education of target's spouse, etc.) so as to cover a broad range of socioeconomic and demographic characteristics. The task for informants, explained to them as a game, was to get a message to the target through a chain of persons. The basic rule was that only persons who knew each other would be allowed to pass the message along. The definition of a "known" person was "someone whom you would feel comfortable asking to deliver a message to the target or some link in a chain to the target." No messages would actually be passed; all we wanted was the choice.

Informants were asked to make a "choice" for each target. A "choice" was defined for them as someone *they* knew who might know the target or might know someone who might know the target. They were told that they could think as far into such a chain as they liked in order to make a choice of a first link to the target from among persons they knew.

Informants initially knew nothing about the targets. We told them that they could ask us anything they wanted to know about each target. The idea was that these questions would elicit the information they needed to make their choices. In explaining this procedure to informants, we told them: "After you have asked questions, you will have a set of information about the target. Try to think of associations (whatever you think an association might be) between the target and someone you know. You will probably want to choose the person you know who is somehow 'associated,' by your definition, with the target." (For a complete description, see Bernard, Killworth, and McCarty 1982.)

Targets' dossiers grew as the experiment progressed, because informants asked questions we had not anticipated. The relevant information was made up on the spot by the inter-

viewer and added to the dossier. As each question was asked, it was recorded in sequence. When informants felt they had asked enough questions, they made their choices. Then they told us why they made those choices (e.g., "because she is a nurse in Atlanta"). Next, informants ranked the questions they had asked in the order they had found them helpful for making their choices.

In our previous study (Bernard, Killworth, and McCarty 1982), informants had asked 82 different questions. Of these, 4 (target's occupation, location, age, and sex) had accounted for more than 50% of all questions ever asked. However, the answers to 2 questions (occupation and location) had been overwhelmingly the most helpful to informants in making a choice, accounting for 64% of the questions judged "most helpful." The target's hobbies and organizational memberships had accounted for a further 11%. The current study replicated this result. Target's occupation was asked on 97% of all occasions, location on 92%, sex on 52%, age on 48%, marital status on 45%, hobbies on 42%, and organizations on 24%. Location, occupation, hobbies, and organizations accounted for 69% of the questions judged "most useful." In the case of targets outside the United States ("foreign targets"), the dominance of location (72% most useful) and occupation (21%) was even more noticeable. Thus the same basic seven questions emerged as in our previous studies. Informants needed to know the following information about each target: location, occupation, hobbies, organizations, age, sex, and marital status.

## THE MAIN INSTRUMENT: THE REVERSE SMALL WORLD

These results showed that informants in Gainesville, Florida, needed to know the same things about targets as did informants in Morgantown, West Virginia (cf. Bernard, Killworth, and McCarty 1982). This enabled us to create a more formal instrument for the main experiment. The survey instrument, which we call the reverse small world, consists of information about 500 mythical targets. Of these, 400 live in various locations around the world and 100 in the United States. We consider the 400 targets the fixed part of the instrument, in the sense that they will be used again and again; however, the information supplied will vary across experiments depending on the appropriate INDEX results. In further, cross-cultural experiments, the 100 U.S. targets will be replaced with 100 targets from the country of the informants.

The construction of the targets had two stages. First, we worked out a distribution of targets' locations, occupations, ages, sexes, hobbies, and organizational memberships. (The omission of marital status from this list was an oversight. Its degree of usefulness was marginal in the Morgantown study and only slightly less so in the current study.) For locations we divided the world into 10 areas: North America, South America, Western Europe, Eastern Europe and the U.S.S.R., North Africa and the Middle East, sub-Saharan Africa, Sino-Tibet, Southeast Asia, Indian subcontinent, and Austronesia. For each country used within an area we assigned two targets, one rural and one urban, one male and one female, one high-status occupation and one low-status. Countries with very large populations (e.g., U.S.S.R., People's Republic of China) were assigned two targets per region. For occupations we began with the Duncan (1961) Occupational Index, which uses a scale of 1 (professionals) to 7 (laborers), and assigned each target an occupation level so as to represent this range. Ages of targets ranged from 20 to 70, with equal numbers in each decadal range. Half the targets were male and half female. Either zero or one hobby and organization membership was assigned to each target.

Second, we created the targets. For each target we had specific numerical information (e.g., occupational level 3, one

NAME	LOCATION	OCCUPATION	AGE	SEX	HOBBIES	ORGANIZATIONS
Marcelina Catalan	Buenos Aires, Argentina	Accountant	46	F	Swimming	Economics Graduate Association
CHOICE _____			F/A	R/B	R/M	
Dhirendra Nath Misra	Calcutta, West Bengal, India	History professor	46	F	China study	K.R. Cama Oriental Institute and Library
CHOICE _____			F/A	R/B	R/M	
Sergey Boytaov	Vardenik, Armenia, USSR	Orchestra conductor	56	M	Collects tapestries	Academy of Arts of the USSR
CHOICE _____			F/A	R/B	R/M	
Jack Kessler	Plymouth, New Hampshire, USA	Band saw operator	58	M	Mushroom collecting	Living Bank (Donor)
CHOICE _____			F/A	R/B	R/M	
Arthur Combes	Bella Coola, British Columbia, Canada	Road grader	48	M		
CHOICE _____			F/A	R/B	R/M	
Mavoia Jonati	Korovou, Fiji	Job tracer	49	F	Dance	Suva City Library
CHOICE _____			F/A	R/B	R/M	
Widad Sayfu	Salug Atiq, Syria	Sugar beet farmer	46	F		
CHOICE _____			F/A	R/B	R/M	
Henri Dobozeni	Niem, Central African Republic	Roofer	28	M	Photographer	
CHOICE _____			F/A	R/B	R/M	

FIG. 1. A typical page of the instrument.

hobby, decade 41-50) from the above schema. To these details we added a realistic name, a specific location, and so on, using common sense to override inconsistencies. For example, we needed an urban target from Latvia, U.S.S.R. We chose the city of Riga from a map. The target, a female in this case, needed a name. Lists of government officials provided first and last names, from which we chose the combination Magdalene Belyak. The schema called for a level-7 occupation for this numbered target, here granary laborer. The target was assigned the age 42 and the single hobby of street beautification. Most hobbies had to be invented (the *Guinness Book of Records* was most useful here), while many organizations were taken from references.

The instrument could now be prepared. The 100 U.S. targets were distributed evenly (every fifth target) throughout the randomly ordered 400 world targets. Figure 1 shows a typical page of the instrument. Informants were asked to write the name of their choice for each target in the space provided. They were also asked to rank the target information according to its usefulness in making their choice, leaving any unused information blank. Finally, they were asked to indicate whether their choice was a friend or acquaintance (F/A), a relative by blood (R/B), or a relative by marriage (R/M).

## THE EXPERIMENT

Informants were solicited through local newspaper ads. The 40 who responded came to our office to take the test but were under no time pressure. Between four and six hours were required to complete the instrument, and informants were paid for their participation. It proved impossible to select informants on criteria other than their age and sex. Their ages ranged from 20 to 66, with an average of 36, s.d. 13; 55% were female. Other socioeconomic and demographic information about the informants is given in Appendix 1.

After an informant had finished the survey instrument, a card was prepared for each different choice made. At this point the informant was required to specify each choice's sex and how he or she "knew" the person chosen. For this latter judgment we defined 12 ways of "knowing," among them having served together in the military, having gone to school together,

working together, and being "just friends." We also asked informants for their views on abortion (33% for), gun control (35% for), disarmament talks (8% for), and the Equal Rights Amendment (30% for). Rather surprisingly, their views on these issues turned out to account for significant amounts of the variance in our findings.

## RESULTS

### NUMBER OF DIFFERENT CHOICES

In our previous experiment (Killworth and Bernard 1978), some informants had used over 800 different choices to initiate 1,267 chains of acquaintance. In the current experiment we provided only 500 targets, and therefore we could not expect the list to elicit the names of all the people our informants knew. However, the number of different choices generated by an informant ought to be a measure of (and perhaps proportional to) the size of that informant's network. The mean number of different choices used was 134, s.d. 65. Figure 2 shows the distribution of the number of different choices used. The shape of the distribution is similar to that found in the earlier study (Killworth and Bernard 1978: fig. 2). In both cases the distribution is bimodal,<sup>1</sup> and there are strong indications that some Americans have very large networks and some have very small ones.

All informants generate a choice for the first target presented, so the mean number of different choices for that target is 1. Almost all informants make a different choice for the second target, so the mean number of different choices for the first two targets is just under 2. As the number of targets increases, informants tend to use previous choices again. Thus the mean number of different choices increases more and more slowly as the number of targets increases. Figure 3 shows this effect. Had we provided enough targets, the curve would even-

<sup>1</sup> These were, however, at different values of network size because of the different numbers of targets involved. Attempts to scale the earlier data to fit the current experiment quantitatively were unsuccessful.

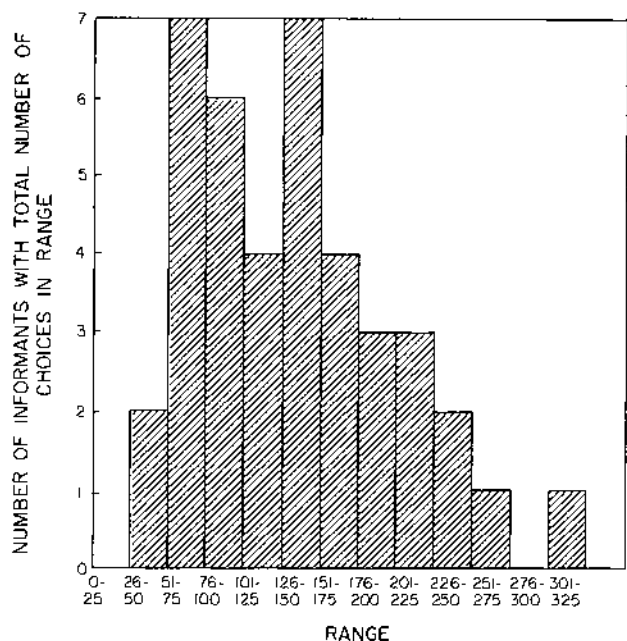


FIG. 2. Numbers of different choices made by informants.

tually have become asymptotic to a constant value. This value could be interpreted as the mean total number of people that this experiment could elicit. It is probably around 250 (see Killworth and Bernard 1978) but is certainly more than could be generated by 500 targets. The dashed line represents the data that suggested 250 as this mean total. The agreement between this and the solid curve is striking. Not only are the means statistically indistinguishable, but so are the error bars.

In other words, there is evidence that the curve is universal, at least for U.S. informants. Support for this comes from examining a curve generated from the 100 U.S. targets (an approximate replication of the 1979 study, in which most of the targets were from the United States), which lies completely on the Killworth and Bernard curve.<sup>2</sup>

We can look at the change in slope of the curves in figure 3 another way. If each informant used each choice equally often, then the curve would be a straight line. It is the inequality of use of choices (i.e., some choices are more popular than others) that accounts for the changes in the slope. The mean number of choices required to account for a given percentage of the "world" (i.e., the 500 targets) is given in table 1. Only 20 choices are required to account for half the targets and only 2 for 10% of the targets. This pattern is qualitatively similar to that found by Killworth and Bernard but (after rescaling to suit the different list sizes) significantly larger\*\* (that is, at the 1% level). It would be surprising if it were not; since three more pieces of information (age, hobbies, and organizations) are provided, choices can be more specialized than in the earlier study and therefore more are needed to account for the same number of targets.

INDEX suggests one explanation for the differential use of choices by informants. The majority of the information provided, for many targets, did not suggest a "good" match in characteristics. Informants would then use a less precise match (e.g., a doctor for all targets connected with the medical field). These choices were "gatekeepers" who "handled," in some way, large blocks of target characteristics.

<sup>2</sup> Conversely, the curve restricting attention to the other 400 targets (i.e., the main world list) lies slightly under the solid curve and is not shown for clarity. The curve does not seem to fit any of a range of theoretical models, including more complicated versions of those tried by Killworth and Bernard (1979).

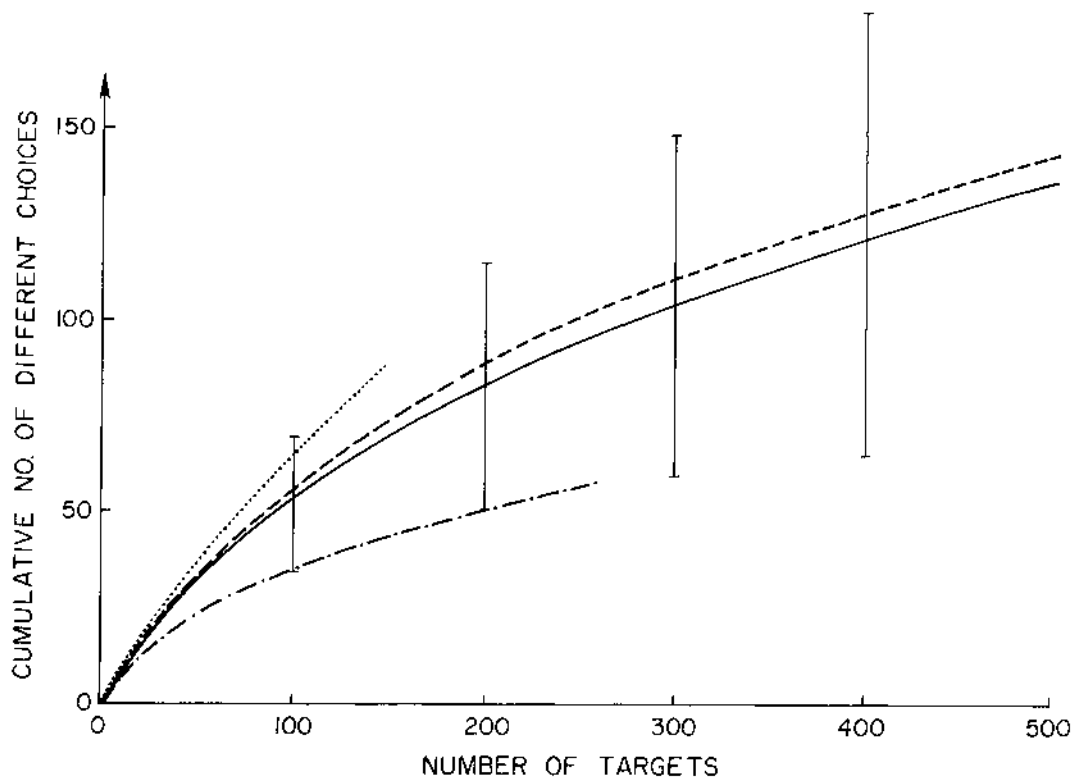


FIG. 3. Cumulative histogram of the mean number of different choices generated as a function of the number of targets presented to informants, with 95% error bars. The dashed line shows the equivalent results of Killworth and Bernard (1978); the dash-dotted line restricts attention to targets for which the choice was made on the basis of location, the dotted line to targets for which the choice was made on the basis of occupation.

TABLE 1  
NUMBER OF CHOICES REQUIRED TO ACCOUNT FOR A GIVEN PERCENTAGE OF TARGETS

	PERCENTAGE								
	10	20	30	40	50	60	70	80	90
Mean .....	2.2	4.6	8.3	13.1	20.0	29.5	43.0	62.3	97.9
S.D. ....	1.6	4.1	7.6	11.6	17.2	24.0	34.1	46.6	60.2

Each informant's top choice (the one used most often) accounted for 11% of the targets; the second-most-used choice accounted for 7% of the targets. (The difference between this and table 1 is due to the different averaging procedures.) These figures can be compared with the 10% and 8% found by Killworth and Bernard; the differences are not significant. Two different lists of targets thus produced virtually the same results with regard to the most frequently used choices.<sup>3</sup>

The wide variation in the number of choices between informants cannot be explained in terms of their socioeconomic status and demographic variables.<sup>4</sup> (We require a conservative 40% of the variance to be accounted for in any correlative fishing expedition for any real predictability.) Informants from rural backgrounds and those against either abortion or the ERA did indeed make significantly larger\*\* numbers of choices than other informants, but at best only 18% of the variance was accounted for.

#### TYPES OF CHOICES

The informants selected their choices from their friends and their family. (We make no further distinction between "friends" and "acquaintances.") Table 2 shows some figures on types of choices. Friends make up 86% (s.d. 7%) of the choices.<sup>5</sup> Although similar in magnitude to the 82% found by Killworth and Bernard or the 80% found by Bernard, Killworth, and McCarty (1982), this is a significantly higher\*\* percentage than either, but Travers and Milgram's (1969) figure of 86% agrees exactly. Of the choices, 64% were male (s.d.

32%), compared with 60% in the 1979 study and 67% in the 1982 one. Thus the basic pattern of about 80-85% friends and about 60-65% males seems to be consistent over several experiments.

As in previous experiments, male and female informants differed significantly in their choice usage. Neither male nor female informants chose more males (82 and 80 respectively); females, however, chose significantly more\*\* females than did males (54 vs. 31). Male informants chose proportionately more\*\* males (71%) than did females (59%).

There was less significant variation in choosing friends versus family. Neither male nor female informants chose more (or proportionately more) friends or family. However, males did choose proportionately more\*\* male friends (64% vs. 51%) and proportionately fewer\*\* female friends (22% vs. 35%) than did females. They also chose fewer\*\* female friends, on average (26 vs. 48).

These findings agree, in all cases, with the results of both earlier studies with minor variations in significance levels. Both sexes chose males over females as intermediaries to the targets, female informants less so. Both male and female informants preferred friends to family, females less so.

Inclusion of other measurable quantities about informants failed to increase the predictability of the types of choices. We constructed multiple regressions using all the information we had about the informants and tried to account for variation in their choosing males over females, friends over family, and so on. At best, we could account for only 25%\*\* of the variance (in the probability of choosing a male). As in the Killworth and Bernard study, few data about informants account for useful amounts of the variance in the types of choices.

Given that most informants tend to choose friends, what kinds of friends are they? As table 2 shows, informants knew their choices, in descending order of likelihood, because they were "just friends" (22% of the time), workmates (16%), neighbors (13%), schoolmates (12%), or had similar hobbies (7%). Other reasons for friendship totalled less than 17% of occurrences combined.

The only category of acquaintanceship that could be explained, statistically, by informant characteristics was "just friends." A linear combination of informant variables accounted for 76%\*\* of the variance of the probability of an informant's choosing "just a friend." The probability increased if the informant favored the right of access to abortion and/or the ERA and if the informant was female. Does this suggest that female, pro-ERA, pro-abortion informants' networks are more populated by "just friends?"

Although we could explain few categories of acquaintanceship statistically, we can report some global findings. For 87% of targets, the most likely sex of choice, defined as the sex which turns up most often for that target, was male. We tried to improve on this prediction by discriminant analysis using the target variables, without success. The most likely relation of choice was a friend for every target, and the most likely sex and relation of choice was a male friend for 85% of targets (cf. 77% in Killworth and Bernard 1978). (In the case of sex and relation, only male friends and female friends ever occurred; family never occurred. Discriminant analysis again failed to improve the prediction, whereas in Killworth and Bernard

TABLE 2  
NUMBER AND PERCENTAGE OF DIFFERENT CHOICES  
BY ACQUAINTANCESHIP CATEGORY

ACQUAINTANCESHIP CATEGORY	NUMBER		PERCENTAGE	
	Mean	S.D.	Mean	S.D.
Friends .....	115.7	56.8	86	7
Males .....	80.2	40.2	64	32
Blood relatives .....	10.2	6.1	8	4
Neighbors .....	17.1	17.6	13	9
Same hobby .....	8.2	11.8	7	10
Workmates .....	16.8	19.4	16	16
Schoolmates .....	14.4	15.3	12	11
"Just friends" .....	25.0	26.3	22	19
Male friends .....	71.0	37.7	51	11
Male family .....	9.2	6.3	7	4

<sup>3</sup> There are two caveats here. First, the list must not contain purely local targets; second, here as in the earlier study we found that a larger percentage (14%) of the non-U.S. list was accounted for by the top choice. Therefore, choices for foreign targets do indeed cover a wider range of targets than do choices for U.S. targets.

<sup>4</sup> This excludes the quantity "How many people do you know?" Although positively correlated with total number of choices, as one might expect, it accounted for only 16%\*\* of the variance in number of choices.

<sup>5</sup> Averaging probabilities over targets, rather than over informants, reduces the percentage of friends to 83%.

[1978] 4% improvement was achieved.) Of the most likely types of friends, 58% were "just friends," although seven categories of friends occurred over all the targets.

Discriminant analyses were also performed on a one target-one informant basis. We wanted to see whether the type of choice could be accounted for if as many characteristics as possible were available for the analysis. As in the Killworth and Bernard study, it could not be. The use of discriminant analysis, for example, increased the predictability of sex of the choice on less than 1% of occasions. In other words, one should always predict a male choice.

#### TYPICAL NETWORKS

For each informant, we counted the number of times that each choice was made on the basis of each of the six pieces of information: location, occupation, age, sex, hobbies, and organizations. We can think of the information found most useful as the "reason" for making that choice (second- and third-most-useful information would correspondingly be "subreasons"). The reason most used defines the "main" use for that choice.

Averaging over all informants produces an average network of 49 (37%) mainly location choices, 53 (40%) mainly occupation choices, 22 (16%) mainly hobbies choices, 6 (5%) mainly organizations choices, 3 (2%) mainly sex choices, and 1 (1%) mainly age choices. The shape of this average network does not depend strongly on how one obtains the information to generate it. For example, if "ever used as a reason" replaces "reason" in the definition of main use for a choice, we find 39% for both location and occupation, 12% for hobbies, and much smaller percentages for others. The dominance—and near equality—of location and occupation as reasons for choice (with hobbies having some effect) confirms the Killworth and Bernard figures of 45% location and 47% occupation. That the latter are slightly higher reflects the absence of hobby and organization information there.

One could count this kind of information in other ways. For example, we might count what percentage of the time each of the six reasons was either the most useful or ever used (which includes the subreasons). Location was most useful 51% of the time (ever used 55%), occupation 26% (32%), hobbies 15% (19%), organizations 5% (11%), sex 2% (2%), age 1% (7%). The emergence of location above occupation, which did not occur in the earlier studies (where the two reasons had very similar usages by any of the measures), reflects the large number of foreign targets in the world list.

Rather than averaging over informants, we can examine individual networks for clues to the way networks are structured. Sometimes a given choice has an unequivocal use (as a gatekeeper, say). For example, one informant's most frequent choice was used for location 99 times, mostly for targets in South America and Eastern Europe, but for occupation a mere 3 times and then only for high-status targets. However, there are more confused situations. Another informant's *fourth* most-used choice was used for location 9 times for targets in various locations; it was used 5 times for occupation for targets with a wide range of occupations.

This effect is shown graphically in figure 3. The dash-dotted curve shows the cumulative number of different choices made for location reasons as a function of increasing number of targets. This curve lies significantly below\*\* the basic curve, thus showing that choices made on the basis of location handle large numbers of targets. Conversely, the equivalent (dotted) curve for occupation, which lies above the basic curve, shows that choices made on the basis of occupation handle very few targets each.

Although characteristics of targets have little predictive ability for the types of choices (i.e., males, friends, etc.), they do predict the reasons for choices quite well. In fact, the occupation level of the target is the dominant feature in determining

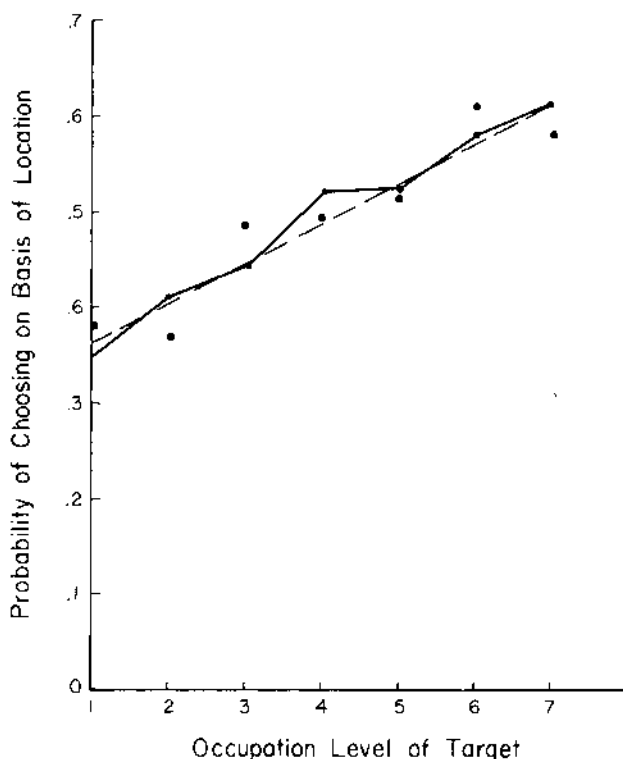


FIG. 4. Probability of choosing on the basis of location as a function of the occupation level of the target. The solid line shows the 400 "rest-of-world" targets; the dots (unjoined for clarity) show the equivalent for the 100 U.S. targets. The dashed line shows the best fit for the rest-of-world targets; 98% of the variance in the world list is accounted for. The best fit for U.S. targets is nearly identical with the best fit for rest-of-world targets.

how likely choices are to be made on the basis of either location or occupation. Figure 4 shows the probability of making a choice on the basis of location as a function of the occupation level of the target. This probability decreases sharply with the status of the target's occupation (recall that occupation levels are coded from 1, the highest, to 7, the lowest). For example, a plumber in Wyoming is likely to elicit a choice based on location, whereas a dentist in Wyoming is likely to yield a choice based on occupation. Target occupation level accounted for 98% of the variance for the targets in the world list (see the best-fit straight line marked in figure 4) and 89% for the U.S. targets. Similarly, figure 5 shows the probability of choosing on the basis of occupation as a function of occupation level of the target. Although the variation about the line is greater than in figure 4, the plausible and opposite trend is visible: as the status of the target's occupation rises, so does the probability of choosing on the basis of occupation. These findings are in complete agreement with those of Killworth and Bernard, except that the distance of the target from the informant has little effect. We assume that this relates to the much larger distances involved with the targets on the world list.

As with types of choices, we can also average over targets to obtain the "most likely reason for choice" for a given target. Of these, 82% turn out to be location reasons. In other words, for a given target, if asked to guess the most likely reason for any informant's choice, one should guess location. However, the analysis above shows that given any choice of any informant, one should guess it to be an occupation choice. This would be correct in 40% of the cases, marginally more accurate than guessing location (37%). The difference between the two cases precisely reflects the specialized nature of occupation choices discussed above.

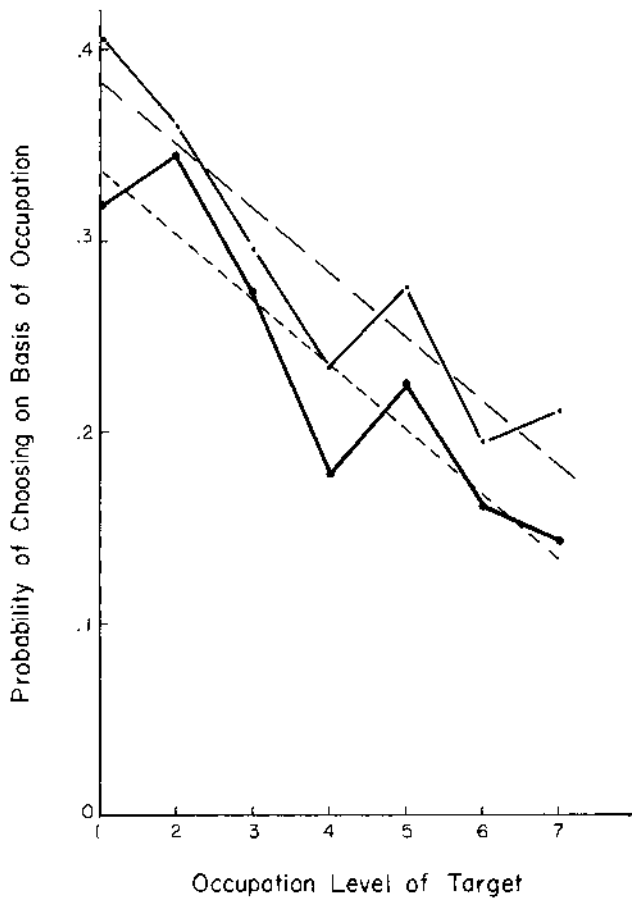


FIG. 5. Probability of choosing on the basis of occupation as a function of the occupation level of the target. *Upper pair of lines*, "rest-of-world" targets and best fit; *lower pair of lines*, U.S. targets and best fit.

#### TOP CHOICES

Because the frequently used choices account for so many of the targets, it is of interest to see how and why this happens. A clear picture emerges if we analyze, say, the 16 most frequently used choices in terms of how often they were used for each of the six possible reasons. The top choice (i.e., the choice used most by an informant) was chosen on average for location reasons 48 times (s.d. 5), occupation 3 times (s.d. 5), hobbies twice (s.d. 5), organizations once (s.d. 4), and age and sex less than once. The second most used choice was used for location reasons 25 times, occupation 4 times, and twice each for hobbies and organizations. The dramatic fall in usage for location reasons of the second most used choice, while the other reasons stay about constant, is a pattern that continues with the less used choices: the number of location uses drops monotonically to about 5 times for the tenth most used choice, while the usage of occupation and hobbies hovers around 2 to 3 and 1 to 3 respectively, at least as far down as the sixteenth most used choice.

The most frequent choices, then, seem to be predominantly made on the basis of the location of targets. Are these target locations evenly distributed around the world, or are they concentrated in one area (or a few areas)? Put another way, do informants rely upon a few important choices for targets in particular areas of the world? INDEX suggests this may be the case. Frequently, informants would establish a choice for an area simply because that person had traveled in that region (for example, a choice who had served in Vietnam would handle the entire Southeast Asian area, together with China, etc.). On average over all informants, the top choice accounted for many

of the 40 targets in each of the 10 areas of the world. The number ranged between 7 targets (in North America) and 16 targets (in China, Japan, Korea, and Hong-Kong). The second most frequently used choice accounted for many fewer targets, from an average of 3 (in North America) to 6 (in Southeast Asia). Subsequent choices decreased rapidly in usage.

Part of this relative uniformity is simply that top choices are used by different informants for targets in different areas. If we examine instead, for each informant, how many targets in any area are accounted for by the top choice, then by the two most frequently used choices, the three most frequently used choices, and so on, and then rearrange the areas in decreasing order of usage, we find that the top choice, on average, accounts for half of one area and nearly half of a second; the two most frequently used choices are sufficient to account for half of each of three areas, five choices for four areas. After this the relevant number of choices increases rapidly. However, we see an apparent specialization of later choices, as is evidenced by the fact that the most-accounted-for area has only 25 out of 40 targets accounted for by the first nine most frequently used choices. In other words, as was found in the Killworth and Bernard study, there is an increase in specialization of the less used choices which here appears as the strong reduction in their location usage.<sup>6</sup>

This discussion recalls the concept of the gatekeeper. Does the amount of an area handled by a gatekeeper (in this case the top choice for that area) depend on how far the area is from the informant? We might expect, for example, that areas relatively close to the informant would be more heterogeneous with regard to choices, whereas areas far away would appear more homogeneous. Therefore the amount of an area handled by its top choice could be expected to be larger for areas far away and smaller for nearer areas. Figure 6 demonstrates this clearly. It shows the fraction of each area accounted for by its top choice as a function of great-circle distance to the "center" of the area from Gainesville. The best-fit line accounts for 41%\* (that is, significant at the 5% level) of the variance and shows the increasing amount of the area accounted for. However, the spread about the line is nontrivial. The two lowest points (North America and South America) have a much smaller

<sup>6</sup>However, the much greater number of foreign targets in this experiment may have modified the type of specialization more strongly towards location than in the earlier study.

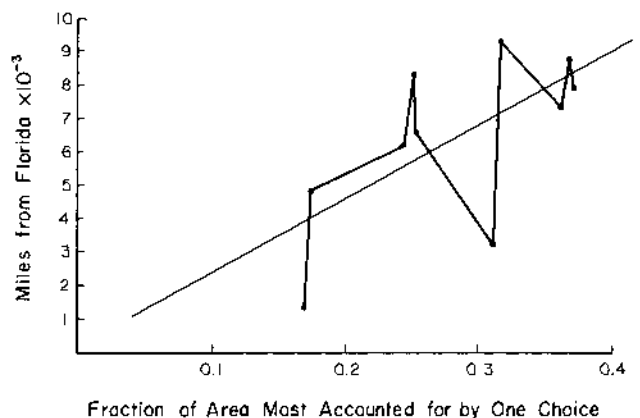


FIG. 6. Fraction of area accounted for by one choice as a function of miles from Florida (in thousands).



amount of their targets accounted for than the other areas. Despite this, choices for targets in far-off areas are not more likely to be made on the basis of location than are choices for targets in nearby areas (only 8% of variance is accounted for).

What kinds of people were the top choices? They mainly reflected the overall pattern of choices discussed above: 76% were males (not significantly higher than the 64% found overall), reflecting the pattern of the Killworth and Bernard study of a preference for choosing males; 82% were friends, of whom 29% were "just friends" and 17% workmates. As in the earlier study, this represents a tendency for family members to be more likely as top choices than elsewhere, but here the difference is not significant. If the main use of each top choice is computed, 33 (83%) are mainly location choices, 3 each (7.5%) are mainly occupation and mainly hobbies choices, and one (2.5%) is mainly a choice on the basis of age. Neither males nor females tended to choose more males as top choices (both sexes 76%), but female informants chose more\*\* family top choices (27%) than did males (12%). (As we have said, however, neither males nor females chose more family overall.) Neither sex used its top choice more than the other.

In looking at the usage of the top choice as a function of its characteristics, only one appears to be important. Female top choices were used more\*\* for location reasons than male top choices (incidentally, an opposite finding from that of Killworth and Bernard). The reason seems to be that the majority of targets in the present study are located outside the United States. Top choices for all targets are infrequently (10% of the time) top choices for only U.S. targets. Hence the inclusion of the foreign targets produces top choices which would not occur for a list of only U.S. targets.

#### FACTOR ANALYSIS

Targets' locations, occupations, and hobbies predominate in "defining" targets for informants. Therefore, we ask: how many, and what, are the basic categories of locations, occupations, and hobbies a target can possess—at least as perceived by informants in the United States? For example, we assume that most informants would perceive all targets from villages in Sumatra as having "similar" locations, namely, "in Sumatra" (if not a broader categorization such as "somewhere around Indonesia").

The categorization made by informants can be found from the principal components (i.e., a factor analysis) of the informants' responses. The data were initially unsuitable for factor analysis. We created similarity matrices  $A^l$ ,  $A^o$ , and  $A^h$ , where the superscripts,  $l$ ,  $o$ ,  $h$ , indicate location, occupation, and hobbies. The matrices are  $500 \times 500$ , each row or column representing one target. They are symmetric, and the  $(i,j)$ th entry is a measure of how similar informants perceive targets  $i$  and  $j$  to be. This measure (e.g., for location) is defined as  $A^l_{ij}$  = number of informants who made the same choice for targets  $i$  and  $j$  and used location as the main reason for choice/number of informants. Since there were 40 informants,  $A^l$  ranged from a minimum of zero (when no informant selected the same choice for targets  $i$  and  $j$  and used location) by increments of  $1/40$ th to a possible maximum of unity (when each of the informants selected the same choice for targets  $i$  and  $j$  and used location).<sup>7</sup>

Each of the three matrices was partially factored. The leading eigenvalues and eigenvectors were found by an iterative method (see Appendix 2). Limits on computer time and storage precluded both direct factoring by more precise methods and further evaluation of eigenvalues/vectors. Because of this we cannot state how much of the variance in the matrix is ac-

counted for by the factors we describe below, but estimates in Appendix 2 indicate that the figure is greater than 90% for location, 80% for hobbies, and 60% for occupation. To obtain the contents of the category corresponding to each factor, we defined targets as belonging to the category if their factor loading (i.e., their element in the eigenvector) exceeded 0.09 in magnitude when the eigenvector was normalized so that the sum of the squares of its elements was unity. Because factor loadings could be both positive and negative, each factor could yield up to two categories, the membership of which would be, in some sense, opposite.

The categories for location are summarized in table 3. Surprisingly, the first factor turned out to be "exotic" locations—locations most people (including one of the investigators!) could not place, such as Nauru, Mali, and the Celebes. The second factor contained a subset of the Iron Curtain countries (Bulgaria, Czechoslovakia, Poland, and the U.S.S.R. but not Yugoslavia, Hungary, or Romania). The entries for the U.S.S.R. were around 0.18, while the entries for the other members of the category were near our cutoff of 0.09. The third factor consisted of Latin American countries (e.g., Bolivia, Mexico, Dominican Republic). The negative loadings on this factor correspond to those for the U.S.S.R.

The fourth category contained India and Bangladesh, with the latter distinguished on the fifth factor. China and Hong Kong were also specified by the fourth factor and African countries (Tanzania, Algeria, and Madagascar, among others) by the fifth. For our informants, *all* of Africa, North and South, was a cognitive unit. Some subtle differentiation became clear in later factors. Southeast Asia, for example, appeared as a monolithic bloc until the sixteenth factor, when Vietnam and South Korea—of obvious relevance to U.S. informants—became differentiated from the rest of Southeast Asia. Similarly, England at first appeared in a category with Scandinavia and Western Europe (but not Italy, curiously) but was later distinguished. Scotland, Ireland, and Wales formed a category of their own.

Figure 7 shows a map of the world with the 16 factors displayed. Readers are left to draw their own conclusions.

Table 4 shows the equivalent calculation for the occupation categories. Again, easily defined and intuitively pleasing categories emerged. The first factor contained suitably high loadings for farming and related occupations, including an alfalfa farmer, an irrigation worker, and a farm-management advisor. This was followed by a category of medical occupations such as doctors, surgical technicians, nurses, dentists, dental hygienists, and surgeons. The third and fourth categories further subdivided the medical field into dentistry, nursing, and medical occupations other than nursing. (Indeed, the three subcategories were all clearly defined even within category 2, dental-related occupations having loadings around 0.4 and 0.5, other medical occupations around 0.2 to 0.3, and nursing 0.1 to 0.15.) Flying and related occupations (including air traffic control) formed a separate category, as did police, jailers, retired parole officers, etc. Mental health occupations were distinguished from other medical occupations by the sixth factor, and the last four factors together defined building/housing, accounting, computing, printing, and a category covering housewives, cooks, and domestic service personnel.

Very clear categories also emerged for targets' hobbies (table 5). Sports accounted for much of the variance, with an immediate subdivision into contained active sports (swimming, soccer, archery, etc.) and free active sports (mountain climbing, cross-country running, etc.) Hunting and other lethal sports formed a separate category, even to the subtle distinction of the inclusion of grouse shooting (which kills things) and the omission of target pistol shooting (which does not). Amateur radio formed a tight category, as did the various forms of collecting things. Painting and sculpting, music, and gardening all formed separate categories. Predominant among

<sup>7</sup> Even for relatively rarely used reasons, such as organizations, entries in the matrix reached as much as  $9/40$ ths.

TABLE 3  
FACTOR LOADINGS OF "SIMILAR" TARGET LOCATIONS

LOCATION	FACTOR																
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	
1. "Exotic" locations	+																
2. U.S.S.R., Czechoslovakia, Poland, Bulgaria		+	-														
3. Latin America			+		+												
4. India				-	+												
5. Bangladesh				-													
6. China, Hong Kong				+		-											
7. Africa					-							+					
8. Vietnam, South Korea						+	-				+			+			
9. Rest of Southeast Asia						+	-				+						+
10. Australia						+	+	-									
11. Canada							+	+	-								
12. New England								+									
13. United States (overall)									+								
14. Western Europe, Scandinavia										+			+				
15. England										+							+
16. Pacific Islands											-		+				
17. Middle East, Cyprus, Turkey												-					
18. Scotland, Ireland, Wales													-	-			+
19. Greenland																	
20. Japan																+	
21. Eastern United States																	-
22. Midwest and Western United States																	+
23. Pakistan																	

NOTE: Loadings are coded by the signs of the factors for which targets in the named locations had loadings of at least 0.09. Limits on computer time prohibited the discovery of further factors.

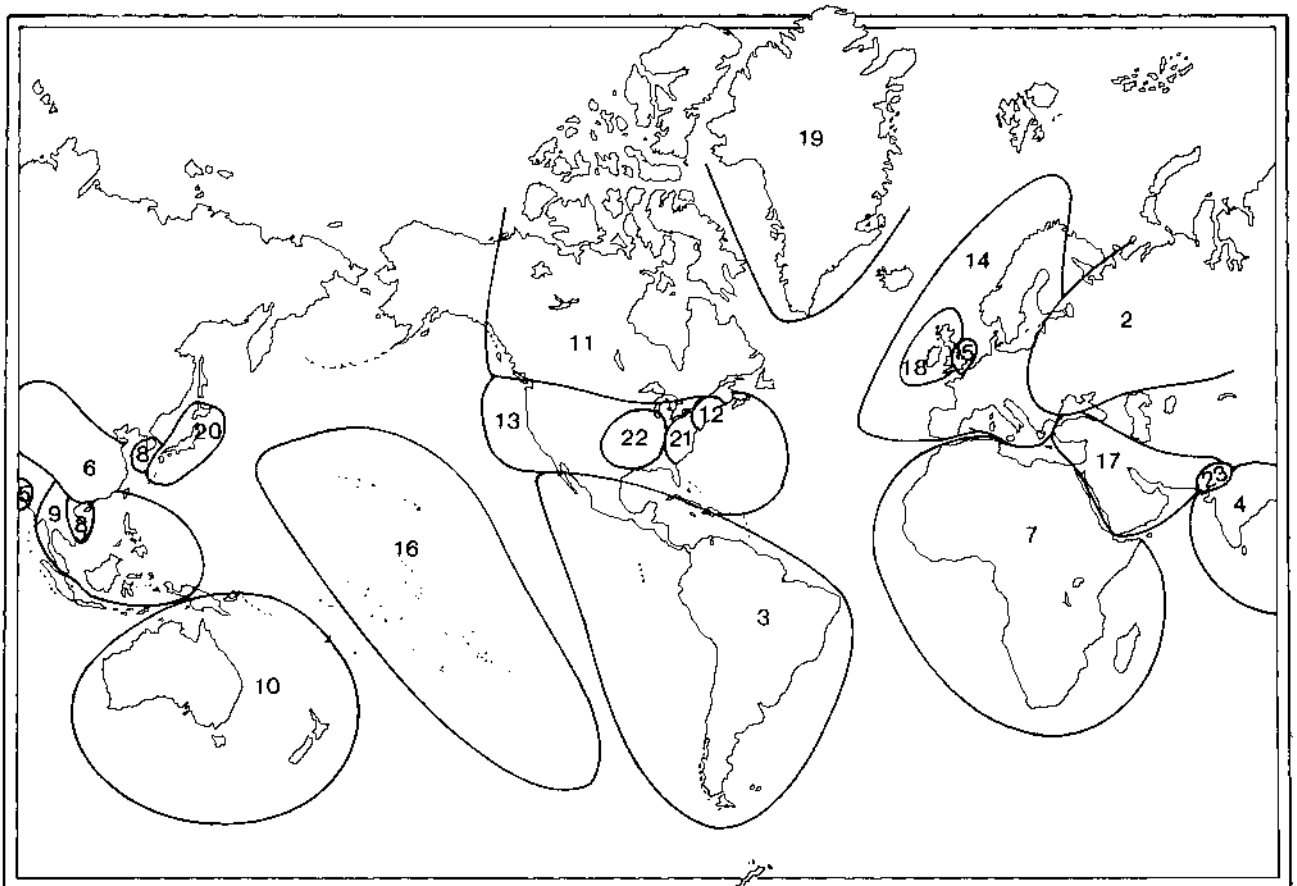


FIG. 7. Areas of the world as defined by factor analyses of informant responses. Numbers correspond to table 3.

TABLE 4  
FACTOR LOADINGS OF "SIMILAR" TARGET OCCUPATIONS

OCCUPATION	FACTOR									
	1	2	3	4	5	6	7	8	9	10
Farming .....	+									
Dentistry .....		+	-							
Medical occupations other than nursing .....		+	+			+				
Nursing .....		+	+	-		-				
Flying .....			+	+	-					
Police .....				+	+					
Mental health .....						+				
Building/housing .....							+	-		
Accounting .....							+	+	-	
Computing .....							+	+	-	-
Printing .....							+	+	+	
Housewife, cooking, maid, etc. ....										+

NOTE: Loadings are coded as in table 3.

TABLE 5  
FACTOR LOADINGS OF "SIMILAR" TARGET HOBBIES

HOBBY	FACTOR								
	1	2	3	4	5	6	7	8	9
Contained active sports .....	+							+	
Free active sports .....	+					-			-
Hunting and other lethal sports .....		+	-						
Amateur radio .....		+	+	-			-		
Collecting .....			+				+		
Painting and sculpting .....				+					
Music .....					+	+		-	
Gardening .....					+		+	+	
Mountain climbing .....	+				+	-			-
Soccer .....	+				-	+			
Fishing .....								+	
Swimming .....									+
Photography .....									+

NOTE: Loadings are coded as in table 3.

the later categories were subdivisions of some of the sports in the first two.

Although organizations were only infrequently used as predominant reasons for choices, and then normally as somehow related to the occupation,<sup>8</sup> we also factored the similarity matrix of organizations. The results, given the less rich data, were predictably weaker. However, clear categories of cultural, library, conservative political, medical, theatrical, forestry, and economic organizations emerged.

The 23 categories of location open up anew the question of top choices or gatekeepers. Calculations equivalent to those in the section on top choices can be performed using the "natural" categories of the world rather than our predefined categories. Figure 8 shows what percentage of each location category is handled by the average top choice for that location. Overall, areas close to Gainesville show a higher degree of specialization of choices; in other words, their top choice handles fewer of the targets in the near locations than does their top choice for targets in the far locations.

We can also consider the notion of a "top top choice." If we array for a given informant all the percentages of targets handled by area, then the maximum such percentage corresponds to the area handled by the top top choice. Figure 9 shows the average of these percentages across all informants, together

with average percentages for the next-to-top top choice, the one below this, and so on. Thus on average every informant has a top top choice who accounts for 79% of one of the 23 areas of the world. For example, one informant may have a gatekeeper who controls 80% of Austronesia, while another may have a gatekeeper who controls 80 or 90% of, say, Japan. The actual areas are irrelevant here, since they vary from person to person. The decline in amount of area accounted for is very gradual; even the twelfth choice accounts for 31% of targets in some area on average.

One thing stands out from these results. They are all immensely plausible to members of a U.S. culture. We take great hope from this observation. If factoring rich similarity matrices is to be a method for obtaining the important categories of reasons for choice for members of a culture, it must satisfy one criterion: It must first prove itself in a situation in which we, as members of a Euro-American culture, know the answers. Thus the apparently trivial findings in tables 3, 4, and 5 are, in fact, highly relevant. Because they are (in some sense) correct for some members of the U.S. culture, we may proceed to use this technique with a modicum of confidence in other cultures.

## DISCUSSION

We have said that three ingredients are essential for the development of a science: description, theory, and prediction. In this research we have developed a method for describing pat-

<sup>8</sup> An example from INDEX will make this clear: One informant explained that she had chosen an acquaintance who was a member of the Sierra Club in order to reach an EPA biologist.

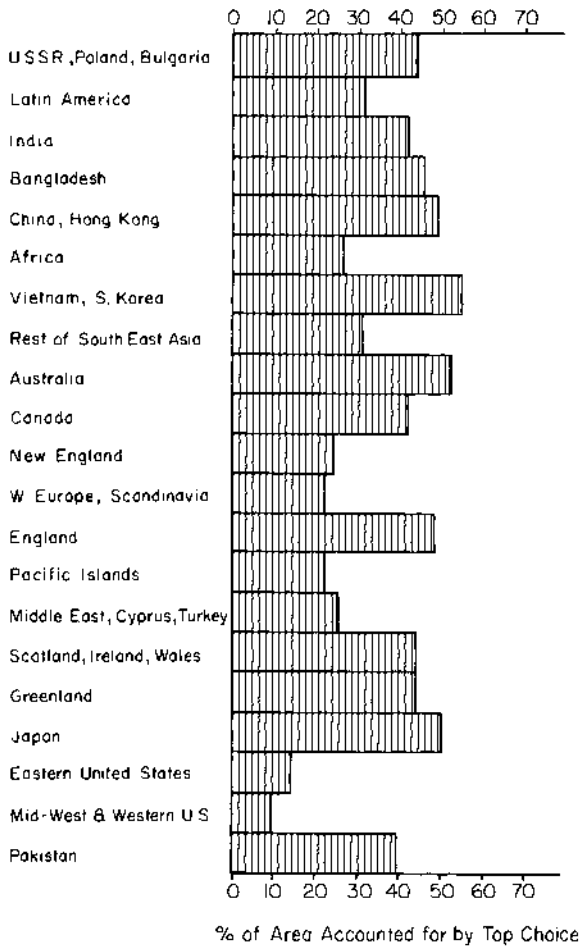


FIG. 8. Percentage of area accounted for by top choice.

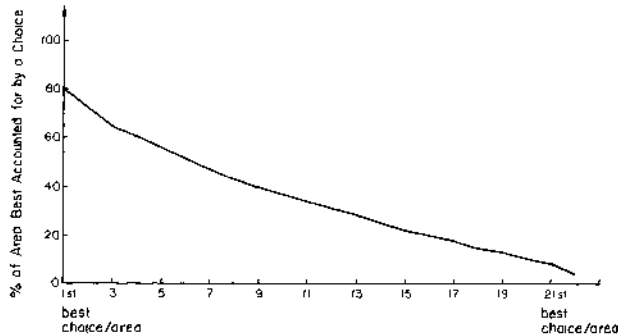


FIG. 9. Percentage of area accounted for by top choice for that area.

terms of acquaintance. Although we believe the results to be of great interest in themselves, their real importance lies in their replicability. Two studies, one in Morgantown and one in Gainesville, have yielded very similar answers. What is more, both replicate findings of Travers and Milgram (1969), whose research methods were quite different (our method analyzed the naming of acquaintances in response to target cues and theirs the passing of folders).

It seems to us that there are four ways to respond to these findings.

First, one might object that the replications were accidental or that another trial in a university town in the United States might yield different results. We reject this objection, since the likelihood of so many detailed replications in our studies is

vanishingly small. We believe, simply, that a further experiment in a U.S. university town would produce almost identical results to those reported here.

Second, one might object that a study in *another* culture (or subculture) might produce different results. Perhaps Kalahari Bushmen might not require targets' locations, occupations, and so on. They might require a different set of information in order to define the intermediaries between them and the rest of the world. This is quite possible and would be interesting to know.

Third, one might object that all the findings are trivial (1) because they are "truisms" (everyone *knows* that location, occupation, etc., are important criteria for knowing other people) and (2) because this complex and expensive (\$20,000) instrument has produced the same results as would probably have been found by a good ethnographer. It may indeed be true that all social scientists in our culture already knew what we have found (though it is unlikely that they know the equivalent findings for Kalahari Bushmen). But consider again our oceanographic parallel. It would seem to be a truism that polar oceans are cold and the equatorial oceans are warm. Still, oceanographers measure temperature in those regions because, as in all sciences, it is understood that intuition is not a replacement for measurement. Precise and accurate measurement *yields* intuition and insight, and vice versa. For example, the high salt content of polar seas is explained by the discovery that freezing sea water into ice produces a lot of salt in the water, as sea ice can hold but little salt. This is not a truism but the result of a great deal of measurement and thinking about data. Similarly, we believe that the findings we have reported are a first step towards understanding what is not obvious about patterns of acquaintance. We certainly hope that a good ethnographer *could* reproduce our findings; indeed, ethnographic confirmation is an important part of studies like this. However, because we have quantified our observations, we have the flexibility to perform many analyses (e.g., the factorings) that yield exciting and nonobvious results. Furthermore, since the replicability, too, is quantified, it is possible in principle to build systematically towards a theory of acquaintance.

Finally, one might respond to our findings by recognizing that, even if our particular method and our particular findings turn out to be entirely wrong, the *approach* we are taking offers the possibility of building towards a theory of social relations.

## APPENDIX 1: INFORMANTS

Twenty-two informants were married, 2 were divorced, and 16 were never married. Twenty-one came from big cities, 12 from small cities, 5 from small towns, and 2 from rural areas. Nine were employed full-time and 10 part-time; 21 were unemployed. There were 7 full-time and 4 part-time housewives. One informant had completed only grade school, 3 high school, and 2 high school and some vocational school; 20 had some college education, 12 had a bachelor's degree, and 2 had attended graduate school. Six had an annual income of less than \$5,000; 12 between \$5,000 and \$10,000; 6 between \$10,000 and \$15,000; 10 between \$15,000 and \$20,000; 1 between \$20,000 and \$25,000; 1 between \$25,000 and \$30,000; and 4 more than \$30,000. Ten were Protestants, 10 were Catholics, 4 were of other (non-Jewish) religions, and 16 had no religious affiliation. Eight considered themselves "ethnic."

## APPENDIX 2. THE FACTORING METHOD

Since the *A* matrices are  $500 \times 500$  in size, computer storage presented a problem. In particular, packaged eigenvalue/

vector routines could not be used. (IMSL and EISPAK, the routines available in the United States, require either all or none of the eigenvectors to be computed and stored, which is prohibitive from the point of view of time and storage. Those available in the U.K., the NAGLIB routines, permit a subset of the eigenvectors to be found; although the main routine runs in 100K bytes, the orthogonalization routine still requires nearly a megabyte.)

To save storage, the matrices were held as half-length integers in a packed (i.e., upper triangular) array, requiring about 250K. Division by 40 could then be made to convert them to the  $A$  entries when necessary. To obtain the eigenvalues and vectors, an initial guess at an eigenvector  $a^1$  was made. A sequence of iterations followed, replacing  $a^{n-1}$  by  $a^n = Aa^{n-1} / |Aa^{n-1}|$ . This sequence converges (Faddeev and Faddeeva 1963) to the leading eigenvector  $b_1$  of  $A$ , with corresponding eigenvalue  $\lambda_1$ , given by  $|Ab_1|$ . To obtain other eigenvalues/vectors,  $\lambda$  and  $b$  are stored and the sequence restarted with  $a^1$ . At each step of the iteration, however,  $a^n$  is made orthogonal to the  $b_i$  ( $i = 1, 2, \dots$ ) by subtracting  $(b_i \cdot a^n)b_i$  from  $a^n$ . (Without rounding error, of course, the  $a^n$  would remain orthogonal to the  $b_i$  forever.) In cases in which convergence is slow (in which  $|\lambda_{i+1}/\lambda_i|$  is nearly unity) the eigenvalues of  $A$  can be shifted (Faddeev and Faddeeva 1963) to improve the convergence, but we never found this necessary.

To estimate how much of the variance was accounted for by the first  $n$  eigenvalues (out of a total of  $N$ , presumably near 500 but unknown), it is necessary to calculate

$$V = \frac{\sum_{i=1}^n \lambda_i^2}{\sum_{i=1}^N \lambda_i^2}$$

An underestimate of  $V$  is obtained by—illegally—replacing  $N$  by infinity in the denominator. Also, the eigenvalues  $\lambda_i$  were observed to decay with  $i$  at an approximately constant rate  $r$  once  $i$  became more than about 5 or 6 ( $r$  varied from one  $A$  matrix to another). Hence

$$\begin{aligned} \sum_{i=1}^{\infty} \lambda_i^2 &= \sum_{i=1}^n \lambda_i^2 + \sum_{i=n+1}^{\infty} \lambda_i^2 \\ &= \sum_{i=1}^n \lambda_i^2 + \lambda_n^2(r^2 + r^4 + r^6 + \dots) \\ &= \sum_{i=1}^n \lambda_i^2 + \frac{r^2 \lambda_n^2}{1 - r^2} \end{aligned}$$

Thus an underestimate of  $V$  is found from estimating  $r$  and substituting into (A2) to give

$$V \geq \frac{\sum_{i=1}^n \lambda_i^2}{\sum_{i=1}^n \lambda_i^2 + \frac{r^2 \lambda_n^2}{1 - r^2}}$$

This gave the quoted figures of 90% for location, 80% for hobby, and 60% for occupation.

## Comments

by PATRICK DOREIAN

*Department of Sociology, University of Pittsburgh, Pittsburgh, Pa. 15260, U.S.A. 10 IV 84*

Bernard and Killworth (1979) ask, "Why are there no social physics?" Their answer is succinct and, I think, correct: anthropologists (and most social scientists, I would add) tend to measure noise rather than signals, and most social processes are dynamic. However, they appear to ignore their own counsel when measuring acquaintanceship. Now, with McCarty, they amplify their answer by exhorting us to be attentive to issues of description, theory construction, and prediction. Yet, while I share their views on these issues, I am puzzled by and disagree with this paper. In terms of constructing theory, I prefer a structural approach to an individualist one. I am a materialist rather than an idealist and seek nomothetic accounts rather than idiographic ones. Though at least one of the authors shares all three predilections, the paper seems at odds with two of them.

In a structural approach, attention focuses on networks and their organization. Killworth, Bernard, and McCarty recognize this. It ought to follow, however, that it is not human behavior (let alone human cognitive processes) but social organization that is the focus of concern. If global social structure is understood in these terms, their research effort goes astray at the outset. Global structure ought not to be viewed as a square matrix (of any size) over a collection of individuals (cf. Mayhew 1980).

Even if square matrices over individuals exist, they are in such a state of flux that sensible measurement may be doomed. Individuals are mired in myriad social networks, switching in and out of them in response to the practical (and frequently material) exigencies of life. If we tried to track this, however, we might measure more noise than anything else, so Killworth et al. were wise to reject the direct measurement of sociomatrices. Once direct measurement of "the" sociomatrix has been abandoned, it makes some sense to seek the rules underlying its generation, but Harris (1975) has provided a strong argument why this will not work either. It is unlikely that knowing categories of thought or even rules for their evocation will provide the basis for understanding human network behavior, let alone global social structure. Further, following Harris (1979), it appears unlikely that a strictly emic accounting of thought processes will help us comprehend global social or network structure. Since the choices measured by Killworth et al. are powerfully constrained, attention could be directed at those constraints. In following Homans (1950) and viewing social structure as built up, even in part, from the relations among individuals, they have backed the wrong horse. A better bet would have been Lenski (1975).

My reaction to their implicit claim that they are concerned with global social structure may render my remarks irrelevant. After all, their title is more modest. If the game is the elicitation of categories of thought and the nodes of an ego-centered network, the reverse-small-world experiment is ingenious. I admire the paper's style and panache, but I have severe reservations about the game. Moreover, I am surprised at their interest.

Perhaps in an attempt to disarm critics, they list possible responses to their paper. Mine is the obverse of the fourth. Concerning the others, I agree that another replication in a U.S. university town would be pointless and that, granting their premises, a duplication in another society (not "culture") would be interesting. Nor should they worry about the apparent triviality of their findings, given their objectives. They ought to worry about the large amount of randomness they report in their data. The parameters of the curve in figure 3

could be of interest as an ingredient in something more global. However, I see their effort as a Houdini attempt to do the impossible by building up from ideas in the minds of people. Houdini surprised and entertained, but, in the end, he tried one trick too many. I fear there is a parallel here.

by SHELDON GOLDENBERG and CLIFF UNDERWOOD  
*Department of Sociology, University of Calgary, Calgary, Alta., Canada T2N 1N4. 7 IV 84*

The discovery that people use information concerning one another's location (geographic) and occupation to begin to establish relationships comes as no great surprise to anyone.

Milgram's small-world experiments are famous not because they first suggested that people used such classificatory schemes, but because they pointed out the shortness of the chain necessary to connect people directly who would not ordinarily have thought they had any way of connecting at all. It was this finding that led to the label "small-world." The second major element of importance in Milgram's work is the empirical charting of real networks, both complete and incomplete, that actually and demonstrably connected people to one another. From such data one can examine differences between networks. Are complete ones more likely to exhibit certain positions as key nodes? Are they more uniform in some respect? Are they more close-knit? Do they exhibit sponsorship, mentoring, or funneling? Are incomplete ones more likely to cross racial or gender or socioeconomic status lines? Are they more loose-knit? One can inductively generate further theory, speculating as to the conditions that characterize and generate each type of network.

It is impossible to derive any of these kinds of empirical network distinctions from the present data. Rather than building on Milgram and the many successors who have fruitfully developed an active subdiscipline of network analysis, Killworth et al. have chosen to concentrate on sophisticated mathematical handling of one very small and possibly rather insignificant detail.

The authors attempt to deal with Milgram's target variability problem. Unfortunately, they give up virtually all of what are particularly the strengths of the small-world technique in the process. Further, in spite of their own earlier concerns not only with target variability but with starter variability, the present research uses 15 paid informants from Gainesville, Florida, to create the formal instrument and 40 more paid volunteers for the main experiment. While no sampling claims are made for these groups, it is clear from their descriptions that they are in no way representative samples of any intended populations, and external validity claims cannot possibly be made on a sampling basis.

Criticizing the statistical treatment in this study would amount to allowing its authors to trap us into formulating our criticisms only within limits they have set. Fundamentally, criticisms concerning their statistical manipulations are not germane, for the major flaw is not analytical; it is theoretical, and no number of factor analyses or discriminant analyses can correct or conceal it. The authors themselves consider their study to be essentially descriptive rather than theoretical or predictive. And indeed, we find no theoretical rationale provided for asking respondents for their views on abortion, gun control, and so on, and no attempt to explain theoretically the use of such variables in the analysis. Throughout the paper, in fact, variables are introduced for no apparent reason other than that the authors can then describe the amount of variance such variables may account for or fail to account for.

The authors claim "to have developed a method for describing patterns of acquaintance." Their findings are that their 40 subjects in a hypothetical experiment nominated others as first steps on a potential route to targets on the basis of location and

*Killworth, Bernard, and McCarty: MEASURING ACQUAINTANCESHIP*

occupation primarily. They acknowledge that "one might object that such findings are trivial." They defend their findings on the basis that reliable measurement is a prerequisite for further development of the area. We would contend that this research fails to accomplish its own announced ends. It does not describe patterns of acquaintance. It reports answers to an instrument attempting to elicit hypothetical first nominated contacts in a laboratory experiment only. First nominations are not acquaintance patterns. Still less are they completed (or even incomplete) networks leading to targets. And a hypothetical experiment with nominated contacts falls far short of the empirical work carried forth by Milgram and the many subsequent researchers working in social network analysis.

In summary, the present work seems seriously deficient in theory, conceptualization, and design, and the sophisticated statistics in our view are simply insufficient to salvage much of merit in that they are fundamentally misplaced.

by PETER HARRIES-JONES  
*Department of Anthropology, York University, Downsview, Ont., Canada M3J 1P3. 15 IV 84*

In the literature on social networks, Killworth and Bernard have taught network analysts during the last decade how to observe, what to observe, and how to devise experiments in measuring communication proximity. Nevertheless, throughout their writing has been a presumption that in measuring communication proximity, social network analysts are also measuring fundamental units of social structure. Killworth and Bernard have said before that whatever structure in a system is, it is composed of an overlap between people's personal networks. In this article they go beyond this to argue that their research aims to produce a theory of social structure based on replicable measurement.

At the outset of postwar interest in network analysis, the aims were more modest. Bott (1957), for example, came to the startling conclusion that the sociological literature had drawn the boundaries between the nuclear family and society-at-large incorrectly. The literature misread the setting or matrix within which family activity took place because sociologists had convinced themselves that modern industrial society had broken kinship ties by progressively reducing "kinship activity" to the nuclear family. Bott redefined "relation" by substituting for the notion of "blood relation" the notion of "relation" as a social connection. By this means she was able to discuss social relationships of the nuclear family in terms other than "kinship ties."

Bott's work suggested a density matrix of social connectedness. In the hands of the Mitchell (1969) group of scholars, this matrix became measurable in three ranges, which have been fundamental to network analysis ever since: (1) proximity, the degree to which individuals' personal networks overlap; (2) linkage distance, the shortest path linking individuals; and (3) patterns of links and non-links within the matrix exhibited through correlations of similarity or dissimilarity. Yet these three ranges are far from being a measurement of social structure. To the contrary, Mitchell, among others, made it clear that networks express social relations in a dimension quite distinct from that of "structure." The pursuit of social relations at the level of personal networks was necessary precisely because methods of structural-functional analysis had become moribund in the context of construction and reconstruction of social ties in urban settings.

Ever since, the distinction of levels between personal networks and social structure has been ignored. As with Killworth, Bernard, and McCarty, networkers have tried their hardest to bootstrap the network dimension to the dimension

of social structure or to paradigms of communication. Personal networks are bootstrapped to these other levels through a two-step assumption. In the first step the topological structure of statistical clusters in lattices or matrices is assumed to be isomorphic to "the structure of social relations." In the second step, through an unwarranted rule of commutation, mathematically structured relations are substituted for "social structure" as if the result of this substitution brought about no change in interpretation of sets of relations.

In my view the first assumption is dubious because the structure of social relations must include far more than a measurement of social space or proximity. The second assumption is accurate only if one holds that the complex ordering of different levels of social experience is reducible to the communication links between its fundamental units. This may well satisfy the communications engineer but is unlikely to satisfy many social scientists. The distortions and dangers inherent in the communication engineer's viewpoint are well illustrated in a recent book on the command and control of United States nuclear forces (Bracken 1983). Where network links are confused with a structure of decision taking and linkage patterns become *de facto* the structure of command, the hope of a controlled response in crises becomes more and more remote.

The patterns of acquaintanceship described in this paper are, therefore, about the *practice* of communication, not about social structure. The authors have shown that in experimental situations of sending messages to a target, the coding operations that informants undertake yield significant information on the categorization of social space in industrial society. Informants' strategies reveal that once the constraints of the code (occupation, location, hobbies, etc.) are known, an important part of success is to match the social position of the target with the social position of the messenger. Hence males are chosen over females and friends over relations.

Strategies of informants cover the goal of the activity in relation to particular places and particular ways of linking space. But what of tactics, the operation of targeting through a variety of means, and what of articulating the message? In the *practice* of communication it could be expected that each would be a level of activity requiring its own analysis and that strategy, tactics, and articulation could not be merged into a single measure of proximity of communication.

by R. M. KEESING

*Department of Anthropology, Research School of Pacific Studies, Australian National University, Canberra, A.C.T. 2000, Australia. 24 III 84*

The methods and results are interesting and the replication of patterns in successive studies very striking, but this paper, for all its pretentious scientism about oceanography, seems very light on theory. The hope that an empiricist, inductivist network-based approach will tell us interesting things about "social structure" seems curiously innocent in the light of modern social theory, Marxist, structuralist, and "post-structuralist." And we should surely by now have gone beyond talking about "members of a culture."

The most serious problem would emerge if the authors attempted to use their method with the Bushmen, who serve as a rhetorical foil in their conclusions. One wonders what a Kalahari subject would do if confronted with granary laborer Magdalena Belyak from Latvia as target. A test instrument intelligible in the Kalahari would have to be constructed in terms of the categories—spatial, sociological, economic—relevant to the Bushmen; that is, a "good ethnographer" would be needed not to *replicate* the findings but to build them into the test instrument in the first place in the creation of culturally appropriate targets and the definition of the task in culturally appropriate terms.

by JOHN SKVORETZ

*Department of Sociology, University of South Carolina, Columbia, S.C. 29208, U.S.A. 9 IV 84*

The idea of social structure as a network of social relations among individuals is a familiar one to most anthropologists and sociologists. The aim of theory is to explain such patterns as exist in these entities. In this context, the research of Killworth et al. purports to describe features of networks that would, at some point, require explanation. The theoretical significance of their results depends greatly on whether the patterns detected by their methodology are induced by "real" principles that govern the formation and change of people's networks or whether they are an artifact of the methodology employed. The fact that several replications yield consistent results does not help decide the question, since such consistency could be obtained from either source.

The conservative position is that the results tell us little about actual networks but a lot about individual behavior in a type of choice situation. To explain the results of this study, then, one would need to adduce what amounts to psychological principles of choice behavior. For example, clearly location and occupation are much more useful than other types of information in helping an informant make a choice, but this is to be expected because knowledge of either of these two carries more information (in an information-theoretic sense) than, say, knowledge of the target's sex: the greater the potential variety of messages from an ensemble, the greater is the information carried by any one message, that is, the greater is the reduction in uncertainty upon receipt of the message. The task facing the informant is to reduce the uncertainty in choosing someone in his or her network to pass on information to a target, and thus knowledge of target's location and/or occupation is likely to reduce this uncertainty more, and hence be seen as more useful, than knowledge of target's sex. A similar argument may apply to the finding that choices based on location are more likely the lower the status of the target's occupation: if high-status occupations have fewer incumbents than low-status ones, knowledge that a target's occupation is high-status serves to narrow the range of choice to a greater degree than knowledge that the target's occupation is low-status. In the latter case, additional information, such as location, is required to reduce uncertainty in the choice of an acquaintance.

While some of Killworth et al.'s results can perhaps be accounted for by simply the structure of their methodology, others may reflect the generic effects of the way relational patterns are actually structured. How general these effects may be is difficult to say, since the informants number only 40 and, as a sample, have some unusual features—over 50% are unemployed. With such a small sample of unrepresentative characteristics, it is not surprising that their attempts to explain findings have few solid results and some unusual ones (e.g., the importance of attitudes toward abortion or ERA). On this basis alone, their results must be regarded tentatively. The authors implicitly recognize this, since they concentrate on reporting their findings and rarely stray into attempts to explain them.

by MONICA VON SURY WEMEGAH

*clo Institute of Advanced Studies in Public Administration, Université de Lausanne, Centre de Recherche, Avenue de Provence 22, 1007 Lausanne, Switzerland. 13 III 84*

The following comments are made by a political scientist unfamiliar with—though I hope not totally ignorant of—both the discipline and the research methodology involved in this article. My remarks will therefore be of a rather general nature, as I am unable to dig into the authors' methodological intricacies. Yet, looking at the journal's "Information for Authors" I must say that from my point of view the authors have not succeeded

in "using a language which can interest those who are not familiar with a specialty." Although I concede that methodology may influence, to a certain degree, the language we use to describe our scientific findings, I still believe that this article could have been written in a much simpler style, using much simpler words, without losing any of its "scientificity." Shouldn't the "sciences of man" that anthropology refers to make a special effort to communicate with their central object of investigation—man or wo-man—not only across subdisciplines but also across the boundaries of other social science disciplines equally concerned with human beings?

Despite these barriers to understanding, I shall venture a few comments on the content of this study. In my opinion, the authors' question "Who knows whom and how?" has not been satisfactorily answered. It is true that, to judge from the title of the article, this may not have been the main objective of the authors, apparently more concerned with the presentation of a methodology that would lead to answering that question than with the answer itself. Nevertheless, I think the study could have profited from a closer examination of the concepts involved here, especially the "how" and the "know" and the relationship between the two. If I understand the writers correctly, the question "how" refers to "what informants find pertinent to their relations with others." My question is whether one can really say conclusively that one knows another person on the basis of knowledge about his/her occupation, location, sex, age, etc. Does a person know many people simply because he has a very large network of personal relationships? The research we have conducted on dominant and alternative ways of life in industrial countries (Miles et al. 1982, Siciński and Wemegah 1983) tends to prove the opposite. In the Western world people, despite their tremendous knowledge of one another in terms of the authors' categories (hobbies, organisations, occupation, etc.), feel more and more isolated from and ignorant of one another and even of themselves. Similarly, though a person may enjoy a large circle of friends and acquaintances, he often has no one to turn to in times of deep personal crisis or despair. Hence the rising rates of suicide, gratuitous criminality, alcoholism, and drug consumption in our societies, not to speak of the blind plunge into consumerism. In other words, I doubt whether people can truly know each other on the basis of such formal categories of information as are used here. From our own research experience, I would further question the applicability of the present methodology in a non-Western setting, not only because it presupposes literacy and a certain level of formal education but also because it operates with thought categories that may turn out to be inappropriate if not irrelevant in many non-American cultures.

I am persuaded that much of this article is also worth commending, but this I leave to the scholars competent in the field. For my part, what I regret most is the inaccessibility of this study to readers outside a narrow specialist circle. A "vulgarisation" of its content, be it only in the form of a summary, would be most welcome.

## Reply

by PETER D. KILLWORTH, H. RUSSELL BERNARD, and CHRISTOPHER MCCARTY

Cambridge, England/Gainesville, Fla., U.S.A. 30 v 84

Each of these comments will require a direct answer. First, however, we will lay out more explicitly the underlying logic of our work and the assumptions about science that our work entails. It is clear that many of the comments are based on assumptions very different from ours, and we believe that understanding these differences is crucial for the development of a social science. We cannot say, a priori, which set of assumptions is correct (and, as we will have occasion to note,

neither can our critics), and for this reason alone we think it is vital that the lines be drawn clearly so that researchers can choose their own path towards unravelling the mysteries of human social interaction.

Killworth is a theoretical physicist, working in the field of ocean circulation. Bernard is a cultural anthropologist who works in Mexico and in Greece. McCarty is a Ph.D. student in cultural anthropology. Killworth and Bernard have worked together for 12 years on problems of common interest, including the search for the rules that govern the formation of social structure—that is, a *theory* of social structure. In the research reported here, we began with the plausible notion that social structure is based on whom people know and how they know them and devised an instrument for measuring this.

We have chosen in our work to use the word "theory" as it is used in the hard sciences. We prefer the words "hard" and "soft" to "physical" and "social" as adjectives for science because we have no evidence that human beings are fundamentally different from other naturally occurring phenomena in the universe, assertions to the contrary notwithstanding. This does not mean that human beings do not possess some intrinsically interesting properties (volition, for example). It means only that these properties provide no evidence for claims that people's behavior (including their volition, if it comes to that), at least in the aggregate, is not subject to relatively simple laws.

In the hard sciences, a theory is a set of rules which accounts for at least part of what one observes and, ideally, tells one what is not relevant as well. From the comments on our paper, it is clear that the soft sciences use "theory" to mean something rather different and certainly far looser. In the hard-sciences sense of the word, we really have no theory of social structure at all. Indeed, in the fields of ocean or atmospheric circulation theory, it would be ludicrous to require a theory of circulation *structure*. We can make a stab at predicting what the ocean and atmosphere will do (with bad results for the ocean up to now, since we are still learning), but "structure" is simply not a term that is relevant to the job at hand.

In the soft sciences, a theory is a set of cogent, plausible beliefs about the world, beliefs that are not generally based on replicated measurements. Since we are still at the stage of searching for law and theory in the soft sciences, starting out with a set of cogent beliefs is as good a way to proceed as any. Regrettably, such theories have so far failed to give us any predictive power. We are not discouraged by this, since it took weather forecasting about 2,000 years (400 if we count from Newton) just to achieve what are only now pretty good results.

What we have tried to do is to lay some groundwork for the "social forecasters" of the year 4000. Lest this sound too grand, we note that we are almost certainly wrong in what we are doing. This statement should not be considered either self-deprecatory or an excuse. It is simply a statistically reliable statement if one examines the efforts made in the hard sciences over the centuries. Nearly all efforts on nearly everything have been and continue to be blind alleys. But one has to try.

Looking at it positively, though, what are we trying to achieve? In the study of ocean or atmospheric circulation there are literally millions of quantities one might want to measure or observe. The trick is to sort out the important quantities from the irrelevant ones. We now know (for the time being, anyway) that velocity, temperature, pressure, humidity, salinity, and a few other quantities are the keys to building a theory. In the case of ocean and atmospheric circulation, the theory is in the form of a set of predictive equations which (for the weather in the United States) produces forecasts that are 80% correct. These quantities are known to be relevant because they produce such good results and because other quantities (such as ocean color) were measured and do not produce useful results. The latter are interesting and remain worthy of



study (indeed, Franz Boas submitted a dissertation on the color of sea water as part of his doctoral candidacy), but they are of no use in predicting what the ocean will be doing next year. It would be nice (and fun) to do the same thing with social structure—that is, to zero in on the millions of things that are interesting and somehow find the three or four that control and predict what happens.

Two different responses are likely to be evoked from readers of this last statement: (a) "But people are far more complicated than the weather; you can't reduce them to a few equations." As noted above, we see no evidence for this assertion and respond in two ways. First, there are a hundred million more atoms in a cubic centimeter of water than there are people on this planet. Why should that cubic centimeter of water be less complex than people? One may *believe* it to be, but that's different. Second, atmospheric equations don't predict cloud color or millions of other things; they just get on with the job of telling us tomorrow's weather. In other words, the "few equations," which have very complex outcomes (such as tornadoes and tidal waves), are not trying to handle everything around them, just a subset. (b) "But what we know of social theory suggests that there are many important phenomena, so that you'll have a huge number of equations. You can't just assert that because physical science gets away with a few equations, social science can do it too." Again, there are two answers. First, we don't *know* that there are many important phenomena for *predicting things*, because, apart from specific fields such as aggregate voter behavior, we are singularly bad at prediction. Second, we see no direct reason for assuming lots of equations when a few *might* do. Occam knew his business.

The upshot of all this is that we are hunting for things that we ought to be measuring in order to build a theory of the type now taken for granted in the hard sciences. We are looking for the building blocks of theory, such as velocity, temperature, and so on. We choose "knowing" as one of those building blocks, and, as we have said, we are probably wrong. We reached the conclusion that "knowing" might be a building block because we could find no evidence that the soft science of society had yet demonstrated even what "social structure" is. Therefore we began with the notion that each nonrandom set of human interactions (mating, owing money, paying visits, exchanging gifts, talking) produces a structure. Having no way to determine which of these behaviors produces the "real" or the "most important" social structure, we assumed that all structured relations depend upon people's knowing each other in some specified way and that the rules governing how people get to know one another must be important—even if any particular structure turns out not to be.

We are well aware that both our definition of "knowing" and our entire method are biased. (The same is true for hard science; velocity meters in the ocean do not register currents running below 1 cm/sec.) But at least we can replicate our measurements (as we have done and as we are now doing in a study of several other cultures), and this is a powerful advantage when one is searching for tools. That our results are no surprise to members of the culture we have studied is confirmation of the measurement we have made. Whether the measurement is trivial or profound is quite another matter, one that will be resolved only by others' making other assumptions, building new tools, and taking new measurements of new quantities.

Doreian discusses structuralist approaches to networks and claims that "global structure ought not to be viewed as a square matrix (of any size) over a collection of individuals." If structure exists at all, then he is almost certainly correct. A meteorologist would balk at the idea that one might have full information about the atmosphere. He or she might want pictures of the atmosphere or might track storms. Nonetheless, in order to understand the pictures and storms, a meteorologist relies on theory. And the theory is based on units such as the velocity everywhere and the humidity everywhere at a specific

time. Thus even a structural look may require more fundamental units for its comprehension.

Doreian argues that we have "backed the wrong horse." As we said above, he is likely—very likely—to be right. This is not because we settled on emic quantities to measure or because we chose to follow Homans (and Radcliffe-Brown 1934) in viewing social structure as built up from the relations among persons but because, at this stage, almost anything is likely to be a blind alley. There are, indeed, many other lines of inquiry which seem to us equally sensible; if Doreian believes that Lenski's assumptions are a better bet, then we can only urge him to pursue his hunch. It is no crime for us to disagree with opinions about what is important to study.

On the other hand, Doreian's criticism (and that of others) that we have not measured anything that people ordinarily do is well taken and requires several responses.

First, people do not ordinarily do what Milgram (1967) asked them to do, either. Nor do atoms ordinarily split other atoms. But the results of both experiments have proved interesting.

Second, Milgram's ingenious experiment yielded exciting information about the length of chains between unacquainted persons in the United States but little information about the decision process by which folders traveled down the chains. Neither did it deal with the reason for the funneling effect by which folders from many starters reached the target through a small number of penultimate links. Milgram's experiment piqued our curiosity about these things, and we devised our experiments to study them; hence our focus on an emic quantity such as "knowing."

Third, the results of our experiments provide the basis for other experiments about "knowing." We defined "knowing someone" as an informant's claiming to know someone when asked by a researcher. Others may find it interesting to take a different approach, such as asking informants to pass around folders. Still others may choose to observe more naturally occurring interactions in a nonexperimental setting. And yet others may wish to wire up informants with surgically implanted sensing devices. Each of the foregoing methods has its disadvantages and is, by definition, narrow. A choice is a choice, after all. It seems to us that the relevant question to ask is whether a particular experiment produces useful results.

Finally, if Doreian is worried about randomness in the data, he should see raw meteorological data!

Goldenberg and Underwood's definition of "theory" differs from ours. The questions they raise are interesting and relevant to the discussion of social structure in which we are engaged, but we know of no proof that they are any more or less relevant than the ones we asked. In fact, the questions about abortion and gun control were included on a hunch of one of us that the political orientations (conservative or liberal) of our informants might have some bearing on whom they knew and how they knew them. Goldenberg and Underwood are correct that we quite simply threw in those questions. But so what? They did, after all, produce interesting results. We are *looking* for theory; we do not accept assertions as theory, and we do not design investigations on other people's thin grounds. Our thin grounds are at least as good as theirs. But ours are (a) new and (b) replicable. If we *had* a theory of social structure, we would certainly use it.

We are uncertain why Milgram and co-workers' very few and certainly not representative targets are apparently statistically permissible for Goldenberg and Underwood, whereas our vastly larger set of informants and our *huge* set of targets are not. As Goldenberg and Underwood note, *we* make no sampling claims. Our data do contain a number of very strong signals that are interesting. Isn't that enough?

Harries-Jones argues that the "structure of social relations must include far more than a measurement of social space or proximity." An atmospheric storm also (as a piece of atmo-

spheric structure) includes far more than just certain measurements, but it cannot be understood (and certainly not forecast) without those building-block measurements. The "structure" grows out of the rules of atmospheric motion. Rules predict many phenomena which are not explicitly within the rules as stated (i.e., mathematical derivations). That is what a true theory does, and that is why we use the rules. "Storms exist and are wet" is *not* a statement in the equations of motion for an atmosphere, but a statement deduced from those equations.

We do not say, as Harries-Jones suggests, that the "complex ordering of different levels of social experience is reducible to . . . communication links." We do claim that the "complex ordering" is the outcome of rules (probably a very few rules) involving simple building blocks. We believe that one of those blocks is communication links. Who knows? Perhaps they aren't relevant at all. If anyone has any data to disprove our claim that "A knows B is one ingredient of social structure" we would certainly like to see it.

Keesing's first paragraph is answered above in the response to Harries-Jones. His comment regarding the need for questions to be germane to an informant is quite correct and precisely reflects our research design. The point of INDEX (the informant-defined experiment) is to divine exactly what the Bushman would need to know in order to make a first choice. In our research, the Bushman's questions, in search of that first choice, would be culturally germane to him or her and would be collected by a skilled ethnographer.

As it turns out, the Bushman example is unfortunate (or at least premature), since our current data collection procedures require literate informants. There exists, however, an array of very different cultures with literate traditions suitable for examination with our current procedures. We would, of course, welcome comments from readers on how our procedures might be used in nonliterate societies and among illiterate members of literate traditions.

Skvoretz's information-theoretic analogy makes a lot of sense to us. It will be interesting to see if it holds up for other cultures, particularly for relatively isolated, nonliterate ones. It is by no means obvious to us, for example, that location and occupation will be dominant categories for all cultures. In fact, we predict that they will not be dominant everywhere.

We also agree with Skvoretz on his useful observation regarding reliability and statistics. We did not have the grant funds (or the computer power) to handle 1,000 informants but would have loved to. Most experiments in the hard sciences cost vastly more money than most studies in the soft sciences. It may well be that reliable, useful results in the soft sciences require spending an order of magnitude more money per experiment. This is not an appeal for our own research but a general observation. Skvoretz's comment is on the mark.

In response to von Sury Wemegah, we honestly tried to write in an informal language. At every juncture, we discussed how best to define concepts for maximal comprehension; it's just that the concepts require very careful definition. Killworth, as a real outsider to all of social science, not merely to anthropology, asserts that our paper is vastly more readable than most in the discipline. This is based on comments from many readers in physics, chemistry, and mathematics (though,

admittedly, the sample is not likely to be representative!). On her other point, we cannot say conclusively that one knows another person purely on the basis of location, occupation, etc. For predictive purposes (i.e., theory generation) it is unclear whether this matters. Once again, by Occam's razor, we assume that what we can demonstrate does matter, within the confines of our experiment. If von Sury Wemegah believes deeply that other things matter, then we invite her to specify what they are and to conduct the appropriate experiments.

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